

PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Seroprevalence of SARS-CoV-2 virus antibodies and sociodemographic features of pregnant women in Mogadishu, Somalia: A cross-sectional Survey Study
AUTHORS	Sh. Nur, Maryan Abdullahi; Dahie, Hassan Abdullahi; Hasan, Nimca Abdi; Garba, Bashiru; Adam, Mohamed Husein Adam; Mohamoud, Jamal Hassan; Dirie, Najib Isse

VERSION 1 – REVIEW

REVIEWER	Villalaín , Cecilia Universidad Complutense de Madrid
REVIEW RETURNED	17-Jan-2022

GENERAL COMMENTS	<p>I have read with interest the study by Abdullahi et al in which the authors present a cross sectional evaluation of women in two hospital in Somalia in terms of their knowledge about COVID 19 as well as their serological status. The study is of interest considering its local impact. Nevertheless there are some issues that should be addressed.</p> <p>Major comments</p> <p>It is not clear throughout the text what the authors mean by COVID 19, is it SARS-CoV-2? or is it SARS-CoV-2 respiratory infection? The truth is that the risk of acquiring SARS-CoV-2 is similar but younger healthier individuals are at a lower risk of developing symptoms.</p> <ul style="list-style-type: none">- Please include the reported sensitivity/specificity/False positive/false negative rates for the test not only in the disussion but on the material and methods section- Statistical analysis needs further detail. Was normality tested? Was logistic regression used on a univariate or multivariate basis?- There seems to be an objective to link socio demographic characteristics or patients behaviour to covid serological status, if this is the case please state it in the objectives.- I would urge extreme caution between association and causation. A significant p value does not mean causation- There must be a mistake: 93% not practicing handwashing vs 82% regularly handwashing?- Please include a strengths and limitations section <p>Minor comments</p> <ul style="list-style-type: none">- Regarding the abstract and introduction, I would disagree with the authors. It has been proven now that pregnant women are at increased risk of complications from coronavirus infection including an increased risk of ICU admission, maternal and perinatal death.- I would also argue that low mortality rates in Africa have been linked to other factors beyond other infectious disease outbreaks such as lower population mean age, lower life expectancy, lower
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	pre-COVID-19 era '65yr+ mortality rate', and smaller pool of people surviving and living with cardiovascular diseases. - Results: were all eligible women included? - The high rate of cardiac disease is remarkable. Is this normal in your setting? - I would suggest some minor language revision
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REVIEWER	Dagneu, Mulat University of Gondar
REVIEW RETURNED	21-Feb-2022

GENERAL COMMENTS	<p>Covid 19 on pregnant women in Somalia. Comments</p> <ol style="list-style-type: none"> 1. In exclusion of study participants, why do you exclude vaccinated pregnant women since the vaccinated also there was reinfection. It is not clear or would you explain it why do you excluded vaccinated pregnant women. 2. In regard to test kits, it would have been better to use antigen test rather than antibody test. Most antibody test tell us about previous infection but antigen test is current infection. 3. In risk factor part in table 1, I did not see any associated risk factor b/c all the values of Odds ratio (OR) are less than 1 if it is less than 1, it is protective. 4. In table 4, in page 12 ``Do you regularly wear face mask``. It seems wrong analysis, it is better to take the reference is wearing mask because it is less risk. If you take the reference is no wearing mask at the end of the day your recommendation based on this analysis not wearing mask is better than wearing mask for prevention of COVID-19. So, it is better to reanalyze the data and consult stastician.
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VERSION 1 – AUTHOR RESPONSE

Reviewer 1 comments to the author

I have read with interest the study by Abdullahi et al in which the authors present a cross sectional evaluation of women in two hospitals in Somalia in terms of their knowledge about COVID-19 as well as their serological status. The study is of interest considering its local impact. Nevertheless, there are some issues that should be addressed.

Major comments

1. It is not clear throughout the text what the authors mean by COVID 19, is it SARS-CoV-2? or is it SARS-CoV-2 respiratory infection? The truth is that the risk of acquiring SARS-CoV-2 is similar but younger healthier individuals are at a lower risk of developing symptoms.

Response: Dear reviewer, we acknowledge your comment regarding the term used for clarification, in this investigation, we set out to investigate circulating antibodies against SARS-CoV-2 virus infection, otherwise called COVID-19 among pregnant women, and evaluated their risks based on some sociodemographic factors (page 3). Dear reviewer, we were only targeting to see if the participants exposed to the virus or not in this study. However, as you have suggested we will only use the term SARS-CoV-2 virus infection AKA "COVID-19" to eliminate any confusion of terms.

2. Please include the reported sensitivity/specificity/False positive/false negative rates for the test not only in the discussion but on the material and methods section

Response: Thank you dear reviewer. We have included the information requested in the methods section of the manuscript (page 6).

3. Statistical analysis needs further detail. Was normality tested? Was logistic regression used on a univariate or multivariate basis?

Response: Yes, normality was tested and binary logistic regression was mainly used for producing the odds ratio.

4. There seems to be an objective to link socio demographic characteristics or patients behaviour to COVID serological status, if this is the case please state it in the objectives.

Response: Thank you for your kind observation. We have included the stated objective as recommended (page 5).

5. I would urge extreme caution between association and causation. A significant p value does not mean causation.

Response: We sincerely appreciate your critical review and totally agree that statistical significance does not necessary equate to causation. We have taken this into consideration in all our arguments in this manuscript.

6. There must be a mistake: 93% not practicing handwashing vs 82% regularly handwashing?

Response: Thank you sincerely for this important observation. Indeed, it was a mistake, as 93 (53.1%) and 82 (46.9%) are the actual frequency/number of participants that practice hand washing and not practice hand washing respectively (page 12).

7. Please include a strengths and limitations section

Response: Thank you for this suggestion. We are happy to notify you that we have already provided a section for Strength and Limitation of the study (after the abstract) as required by the journal (page 3).

Minor comments

8. Regarding the abstract and introduction, I would disagree with the authors. It has been proven now that pregnant women are at increased risk of complications from coronavirus infection including an increased risk of ICU admission, maternal and perinatal death.

Response: Thank you for your foresight. Indeed, recent reports have demonstrated increased risk of COVID-19 complications among pregnant women as you rightly alluded. We have updated the manuscript to reflect this important observation. Thank you once again (page 4).

9. I would also argue that low mortality rates in Africa have been linked to other factors beyond other infectious disease outbreaks such as lower population mean age, lower life expectancy, lower pre-COVID-19 era '65yr+ mortality rate', and smaller pool of people surviving and living with cardiovascular diseases.

Response: We also agreed that low testing rates is not the only possible reason why COVID-19 rate is low in Africa. We have incorporated the additional factors you pointed out (page 3-4).

10. Results: were all eligible women included?

Response: Yes, all eligible women who consented were included in this study (eligibility was defined as being pregnant, not vaccinated against COVID-19, and attended the hospitals studied during the period of the investigation).

11. The high rate of cardiac disease is remarkable. Is this normal in your setting?

Response: It is difficult to conclude that this is normal without further detailed investigation. However, some studies have reported that risk factors associated with cardiovascular diseases are higher among Somali women (doi: 10.3390/ijerph16132353).

12. I would suggest some minor language revision

Response: Thank you for your suggestions. We have made considerable efforts to improve on the quality of the grammar and corrected all typographical errors

Reviewer 2 comments to the author:

Seroprevalence of SARS-CoV-2 virus antibodies and sociodemographic features of pregnant women in Mogadishu, Somalia. Manuscript ID. bmjopen-2021-059617

The study by Nur et al. raises public health important issue about the magnitude of SARS-CoV-2 and risk factors among pregnant women is vital for Africa specifically in Somalia where COVID-19 prevention is not given much attention due to lack of infrastructure and other related issues. So the manuscript is in the range of BMJ OPEN journal.

However, before publication, the following points must be addressed.

Comments

1. In exclusion of study participants, why do you exclude vaccinated pregnant women since the vaccinated also there was reinfection. It is not clear or would you explain why do you exclude vaccinated pregnant women.

Response: Dear reviewer, we thank you for your time and comments. We essentially excluded vaccinated pregnant women so as not to have false-positive antibody detection due to vaccine antibodies. We also acknowledge that even after vaccination re-infection can occur, however, we still decided to exclude vaccinated women to control any possible bias of the results. Thank you once again.

2. In regard to test kits, it would have been better to use an antigen test rather than an antibody test. Most antibody tests tell us about the previous infection but antigen test is current infection.

Response: This is absolutely correct. However, our objective in this study was not to detect active infection, but to determine the presence of circulating anti-SARS-CoV-2 antibodies, in order to understand the burden of the disease in the population. Accessibility to antigen-based serological test kits would have posed a serious challenge for us.

3. In the risk factor part in table 1, I did not see any associated risk factor b/c all the values of Odds ratio (OR) are less than 1 if it is less than 1, it is protective. Please reanalyse the data.

Response: Thank you for your critical assessment of our manuscript. We appreciate the concern of the reviewer with respect to the results in table 1. Despite modification to the table as suggested, the result was unchanged. This results may have been contrary to many earlier reports, but to the best of our analysis and understanding, this is what we observed from this study. Thank you once again (page 8).

4. In table 4, on page 12 ``Do you regularly wear face mask``. It seems wrong analysis, it is better to take the reference is wearing a mask because it is less risky. If you take the reference is not wearing mask at the end of the day your recommendation based on this analysis not wearing a mask is better than wearing a mask for prevention of COVID-19. So, it is better to reanalyse the data and consult a statistician.

Response: We appreciate the concern raised by the reviewer. Regardless of the reference we used (wearing a mask or not), the odds would've been less than one. We believe the discrepancy may have arisen from the fact that, although 56.2% reported the regular use of mask, this may not have seemed protective because of the carelessness in observing the routine preventive measures with the people are reported to exhibit thinking that the mask will protect them while ignoring other preventive measures. Interestingly, only 25.6% of those that claimed they wear mask reported they used the recommended mask (N95-9% and surgical mask 16.6%). This implies that that the mask the claimed

to have been using may likely be unapproved face coverings like mask made of simple clothes etc. (page 15, end of first paragraph).

Thank you once again dear reviewers and dear editor for this great comments and opportunity. Please do not hesitate to ask us for any clarification or correction required regarding this manuscript.

VERSION 2 – REVIEW

REVIEWER	Villalaín , Cecilia Universidad Complutense de Madrid
REVIEW RETURNED	23-Mar-2022

GENERAL COMMENTS	Thank you for your comments and changes through the manuscript.
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REVIEWER	Dagnew, Mulat University of Gondar
REVIEW RETURNED	12-Apr-2022

GENERAL COMMENTS	<p>Seroprevalence of SARS-CoV-2 virus antibodies and sociodemographic features of pregnant women in Mogadishu, Somalia Manuscript ID.bmjopen-2021-059617</p> <p>The study by Nur et al., raises public health important issues about the magnitude of CoVID-19 and risk factors among pregnant women is vital for Africa specifically in Somalia where COVID-19 prevention is not given due attention due to lack of infrastructure. So the manuscript is in the range of BMJ OPEN journal. However, before publication, the following points must be addressed. Some of the questions were addressed in a good way but the others were not satisfactory again correct the following points</p> <p>Comments</p> <ol style="list-style-type: none"> 1. In exclusion of study participants, why do you exclude vaccinated pregnant women since the vaccinated also there was reinfection? It is not clear or would you explain it why you excluded vaccinated pregnant women. 2. In the risk factor part in table 1, I did not see any associated risk factor b/c all the values of the Odds ratio (OR) are less than 1 if it is less than 1, it is protective. Please reanalyze the data.
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VERSION 2 – AUTHOR RESPONSE

Reviewer 2

1. In exclusion of study participants, why do you exclude vaccinated pregnant women since the vaccinated also there was reinfection. It is not clear or would you explain why do you exclude vaccinated pregnant women.

Response: We totally agree with the reviewer that reports have shown possibility of re-infection among vaccinated individuals. However, the decision to exclude vaccinated women was because the kit we used is not able to distinguish antibodies as a result of vaccination from antibodies due to

actual infection/re-infection. Therefore, including vaccinated women may lead to erroneous conclusion due to false-positive antibody detection (10.1080/17476348.2021.1917389). We have highlighted this point as one of the limitations of the study. Thank you.

2. In the risk factor part in table 1, I did not see any associated risk factor b/c all the values of Odds ratio (OR) are less than 1 if it is less than 1, it is protective. Please reanalyze the data.

Response: Thank you for your observation. We have re-analyzed the table and re-grouped the variables assessed, and have updated the manuscript accordingly.

VERSION 3 – REVIEW

REVIEWER	Dagneu, Mulat University of Gondar
REVIEW RETURNED	09-May-2022
GENERAL COMMENTS	I have no further comment.